materials is elaborately studied; the errors due to occlusions of gases by metallic oxides, and of water by crystallized salts, are pointed out; and by attention to minutiæ of this kind the accuracy of the determinations has been greatly increased.

In general, with a few exceptions, Professor Richards has confined himself to one group of methods, namely, the analysis, by known processes, of metallic chlorides and bromides. These, in nearly all instances, involve a knowledge of the atomic weight of silver, through which the atomic weights of the other elements are referred to that of the standard, oxygen. That is, ratios are determined, from which, with reference to silver as the experimental standard, the other atomic weights are computed. At first, the secondary standard $Ag = 107.93$, established by Stas, was accepted; latterly, however, it has been shown by several authorities that $Ag = 107.88$ is nearer the truth, and that the true value may even be slightly lower. This change produces corresponding changes in the other atomic weights; a condition of affairs which is not altogether satisfactory. In most cases each atomic weight determined by Richards is a function of the atomic weights of silver, chlorine and bromine, and these have been, in effect, three variables. Theoretically they are constants, but the values found for them have varied, and the variations are far reaching in their effects. The great exactness of Richards's work is in the measurement of definite ratios, which, once established, form the basis upon which our knowledge of the atomic weights must stand. As the variations in the reference values diminish, the accuracy of our deductions will increase.

From one point of view it is well that the Harvard chemists should have devoted themselves, not exclusively, but in great part, to one group of methods. Those methods have been perfected, their sources of error have probably been reduced to a minimum, and the measurements made with their aid leave little to be desired. Considered more broadly, however, it is desirable that other, radically different methods should be developed with equal thoroughness. Not until that has been done, not until closely agreeing determinations of atomic weights have been made by several distinct reactions and processes, can we regard these constants as sharply established. Work of this sort, especially with reference to the more fundamental atomic weights, is now going on in several laboratories, among which may be mentioned that of Guye, at Geneva. Within the next ten years our knowledge of the atomic weights is likely to be greatly increased. Meanwhile, the work of Richards and his colleagues must be assigned preeminence.

F. W. CLARKE

Elemente der Exakten Erblichkeitslehre. By W. JOHANNSEN. Deutsch wesentlich erweiterte Ausgabe in fünfundzwanzig Vorlesungen. Jena, G. Fischer. Pp. vi + 515. Gebunden, 10 Marks.

The epoch in evolutionary study opened by deVries's "Mutationstheorie" had been one not only of experimentation, but also, fortunately enough, of thoroughgoing analysis. We had analysis of evolution in sufficient amount, even *ad nauseam,* in the latter part of the last century; but the newer speculations are based on novel, experimentally acquired facts, and the marvel of it is that they bear little resemblance to the conventional and orthodox teachings which we accepted almost without question a decade or two ago. It is to the shame of biological science that it must be acknowledged that it was long contented to accept these speculations as fundamental principles without testing them experimentally. But all that is now happily by and the era of framing hypotheses for the purpose only of testing them is well launched.

Of the old ideas, those grouped about variation have undergone, perhaps, the completest analysis. And they needed it too, for if one thing is clearer than another, it is that Darwin and his followers did not analyze the phenomena of variation satisfactorily. It is almost pathetic to see in his letters and books how he fails to distinguish the fundamental differences between fluctuating non-inherit-

able variations and such characters as serve to distinguish one kind of poultry or mammal from another. To-day we see more clearly that a new character, such as "angora" hair or an extra toe, belongs to a different category of variations from ordinary fluctuations in the length of the hairs on the body of a cat and variations in the thickness of a toe; for a hair will be more or less long according to the nutrition it receives at the base (and this varies at different times), and the toe will be more or less thick, depending on the use to which it has been put. The variations dependent on environment or use are, so far as we know, not inherited, while the new characters clearly are. Thus the primary classification of variations is based on their heritability. This much was pre-deVriesian.

The new viewpoint, introduced by deVries, and extended by Johannsen, affects the interpretation of those slight variations that seem to be independent of environment and are distributed about a mean value in the form of the familiar "frequency polygon." The biometric "school" laid stress on this sort of variation, and held that by selective breeding from the extreme variants through many generations an indefinitely wide departure from a starting point might be effected. This deVries denied, but held that, while such selection might lead to a certain departure from the mode, the degree of such a departure was restricted through a strong regressive tendency. Here Johannsen steps in, analyzing more completely this result of breeding from the extremes of the frequency polygon.

The fundamental principle of Johannsen is that an ordinary frequency polygon is usually made up of measurements of a characteristic belonging to a non-homogeneous mass of individuals; that it is really analyzable into several elementary masses each of which has a "frequency polygon" of its own. In each elementary polygon the variation is strictly due to non-inheritable somatic modifications, selection of extremes of which has no genetic significance. But the selection for breeding of individuals belonging to *different* elementary polygons, lying, say, at the extremes of the complex, may quickly lead to an isolation of these elementary polygons, the constituent individuals of which reproduce their peculiarities as distinct elementary species. Thus Johannsen holds that not only do individuals with qualitatively dissimilar characters belong to distinct elementary species, but often such as are only quantitatively unlike. The complex variation-groups are called by the author *phænotypes,* or false types, the elementary variation groups are *genotypes,* or genetic types.

What is the proof of the existence of these two types? It lies in the author's experiments in breeding "in the pure line." Whenever, in a self-fertilizing species, a character is measured through successive generations it does not show a regression toward the mediocre position of the entire population, but regression occurs only to a near-by mode of the elementary genotype. In such a species regression to mediocrity occurs only when we consider the offspring of parents which, even though similar, belong to distinct genotypes. For, since genotypes overlap, the parents, though quantitatively similar in any organ, may have children that regress in an opposite direction to the modes of their (unlike) genotypes, and thus be quite dissimilar to their parents. In the long run the change from parents to offspring will be in the direction of mediocrity. This is the usual result and it has obscured the facts of genotypes in the midst of, and as constituents of, the phenotypes. Now, although self fertilization is necessary to the *proof* of the existence of genotypes, such types are believed to be universal and necessary to the interpretation of heredity and evolution. So the author in his 500-page book rewrites the science of heredity from the new standpoint.

The book is in the form of twenty-five lectures. The first six are devoted to variability and its statistical analysis; then follow five devoted to selection and regression; three to aberrant and complex frequency polygons; four to correlation; two to types of variation; one to effect of environment; two to hybridization; one to nutrition and one to human

heredity and the theory of the determiner. The field is well covered.

Of the sections dealing with variation and selection it may be said that they contain the sharpest analysis yet made of the biological significance of the variation polygon and of its modification under diverse ancestry and environmental conditions. Considerable space is devoted to the interpretation of skew curves, indicating that, for organisms at least, they are not due to an inequality of the plus and minus selective forces but rather are a necessary consequence of an initial inequality of growth combined with the law of *proportional,* as contrasted with absolute, increments. As to selection, the results of extensive experiments, of which the details are given, indicate that selection can not create genotypic differences. Among abnormal frequencies, bimodal polygons receive fullest attention and several causes are deduced, such as: presence of two races, of two age classes, of two environmental conditions, of dimorphism and of mendelian segregation. In treating of correlation tables the author reaches the conclusion, now generally accepted by modern workers in heredity, that, while useful for many purposes, such tables are useless in the study of heredity in the strict sense.

The general effect of the prolonged argument of the author is to arouse enthusiastic acceptance of the principles he works out, which, indeed, seem in the line of necessary development of modern ideas. Every breeder of experience must have noticed the fact that even trivial, often quantitative, differences may be inherited as unit characters and persistently refuse either to blend or to regress. Such are the genotypes of our author. Nevertheless, the body of heredity data is still so small that we may well hesitate to accept in any other spirit than as a working hypothesis the principles of Johannsen. If it should prove to be possible, in a case where the existence of a biotype-complex can be excluded, to pass by "selection" from one genotype to another, then the value of the hypothesis would be greatly diminished. To this test several scientific breeders are devoting their energies and we shall soon have more data on the matter.

CHAS. B. DAVENPORT

SPECIAL ARTICLES

THE ACTION OF RADIUM SALTS ON RUBIES

IN 1906, Marcellin Berthelot[1] found that crystals of amethyst from Brazil became decolorized when heated to 300°, but that on exposing the decolorized crystals to the action of radium chloride, contained in a sealed glass tube, the original color was regained in the course of a few weeks, owing to the reoxidation of the manganese salt. He suggested that the color of amethyst, and possibly of some other precious stones, may be due to the action of radioactive substances while the stones lie buried in the lithosphere.

The following year Bordas[2] reported that when a blue sapphire is exposed to the action of radium bromide of activity 1,800,000, the color changes to a green, then to bright yellow, and finally to a deep yellow. Under the same conditions, a red sapphire was found to change through violet, blue and green to yellow. Bordas stated that the intensity of the reaction can be varied by altering the distance of the stone from the radioactive salt, or by employing radium bromide of different activity; and concluded that since yellow sapphires are the most common, and blue and yellow ones are frequently met with together, it seems probable that the soil in which these precious stones are found is radioactive, and that the stones are undergoing a very slow change analogous to that he observed. Later[3] Bordas observed that by bringing a tube of radium bromide of very high activity (1,800,000) into direct contact with a corundum, and varying its position every few hours, the coloration can be effected evenly in some days. It was ascertained that colorless corundums can be rendered yellow, and the color of natural topazes and faintly colored rubies intensified in color. Artificial rubies were found to be similarly affected.

[1] *Compt. rend.*, 143, 477.

[2] *Compt. rend.*, 145, 710.

[3] *Compt. rend.*, 145, 800.